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SECTION OF BIOLOGY[1]

PROFESSOR THEODOSIUS DOBZHANSKY, Columbia University, New York, N. Y.: *Races and methods of their study.*

> Will he not fancy that the shadows which he formerly saw are truer than the objects which are now shown to him?
> Plato

Science is not a body of unalterable dogma. It is a growing; changing; at times, senescent; and then, again, rejuvenating organism. Theories and methods start as revolutionary innovations, gain general acceptance, and eventually become impediments to discovery, if applied outside their proper sphere. History of systematics, and particularly of the race concept as a systematic category, are illustrations of this cycle.

There are, in round numbers, 1,100,000 described recent species. Their actual number is hardly less than 1,500,000 and probably more. The human mind is powerless to cope with so tremendous a diversity, except with the aid of the device known

[1] No meeting was held in January by the Section of Geology and Mineralogy. The Section of Anthropology met on January 26. A paper was delivered by Dr. W. Duncan Strong, entitled "Recent Archeological Research in Peru." No abstract was received.

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NOTE: This number contains the A. Cressy Morrison 1942 Prize Contest Announcements.

as hierarchic classification. In biology, this device was first contrived and applied by Linnaeus, and it continues to be applied to our day. We marvel at the scientific insight of Linnaeus who selected species as the basis of classification, the category of species being the only category above the individual, which, as we now know, has a high degree of objective reality. Species are combined in genera, genera in families, families in orders, etc. These higher categories are arbitrary in so far as it is a matter of convenience as to which subdivision is to be regarded a genus and which a family. They are not arbitrary in so far as the dividing lines between them are made to coincide with the actually existing discontinuities among living things.

Species was the elementary group in Linnaeus's system. Below species were only individuals. Very soon, it was noticed that species are, in turn, subdivided into groups of individuals possessing common characteristics. These groups were termed races or varieties. Linnaeus himself was, of course, aware of the existence of human races, four of which he recognized and described. Very presently Buffon, the father of anthropology, was studying human races in more detail. The eighteenth century of Linnaeus and Buffon was the time of encyclopedic philosophies, of belief in powers of Reason, of rapidly expanding knowledge of new lands, and of supreme confidence in the orderliness of nature. There appeared to be no ground why the methods of describing and studying races should differ from those used to describe species, genera, or other groups. Despite many refinements in the methods of systematics introduced since Linnaeus and Buffon, all categories are still treated alike in principle. The methodology of systematics is determined, first and foremost, by the practical need of pigeon-holing the museum materials in a way that makes them available for further study. But, at its pinnacle, systematics becomes an analytical science intent on discovery of basic regularities underlying the observed facts.

To begin with, systematists were occupied with sorting out complexes of specimens which resembled each other more than they did specimens of other complexes. The complexes were

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usually designated species. This stage in the development of systematics has not been overcome to our day in the little known groups of animals and plants and in the insufficiently studied faunas and floras. As more and more abundant materials became available, and as the preliminary orientation work in the better known groups approached completion, more discrimination became necessary. Knowledge of the geographic distribution, variation and ecology, usually, though not always, permits us to decide whether two complexes of specimens represent distinct species or races of a single one. Systematists are seldom able to arrange experiments that would demonstrate directly whether the complexes with which they work are or are not prevented by reproductive isolating mechanisms from exchanging genes with each other. The criteria which advanced systematists are using to distinguish species from races are, however, ways to discover by inference whether such an exchange of genes takes place in nature. We know that in the progression from race to species the evolutionary process accomplishes a biological transformation of utmost importance. Races are open systems that exchange genes or are capable of exchanging them. Full fledged species are closed systems that are debarred from exchanging genes by reproductive isolation. The transition from race to species is gradual. Reproductive isolation takes time to become established. Hence, in some instances, it remains arbitrary whether a population is considered a species or a race. This difficulty is inherent in the evolutionary process, but it does not follow that species, in general, are arbitrary entities. At our time level, some groups have unquestionably reached the stage of separate species and other groups are only racially distinct. Still others are in the process of transition. (No other but purely morphological criteria are, however, applicable, when series of forms succeeding each other in time are compared. Dividing an unbroken chain of forms into species or races is a matter of convenience only.)

Racial studies have been carried furthest on human races, on those of certain domesticated forms, and in a few most favored groups of wild animals and plants. Samples of individuals in-

habiting a certain region are taken; as many of their characters as practicable are examined; and the averages and the limits of variation of these characters are determined. The result is an ideal racial type or standard. Any individual, unless it is a species hybrid, belongs to some one and only one species. It is assumed that any individual must belong to some one race, namely, to that to the standard of which its characteristics are closest. Individuals which show obvious mixtures of characters of two or more races are dismissed as "intermediates," or considered, frequently on entirely insufficient grounds, to be race hybrids. This method of racial studies is evidently similar in principle to that applied for studying species; *i. e.*, species may be described in terms of ideal statistical types toward which the individual variants gravitate. There is no doubt that this method is convenient for a preliminary orientation in the observed diversity of racial variants, as well. It is with the aid of this method that anthropologists have arrived at a general view of the subdivisions of mankind and, to a certain extent, of their historical interrelations. But this method happens to be misleading when used to analyze the nature and causes of racial variability. Its indiscriminate use has made possible the pseudo-scientific theories of racialism which have played so sinister a role in the preparation of the present world cataclysm.

Before the advent of Mendelism, biologists had believed that heredity is transmitted through "blood." Different hereditary "bloods" were supposed to mix even as alcohol commingles with water. If this view were correct, the hereditary endowments of children would be always a compromise between those of their parents. Brothers and sisters would be genotypically alike. Any sexual population, regardless of how mixed its origins may be, would become more and more uniform with time; since the only source of diversity, after a thorough mixture of the "bloods," would be the new variability arising through mutation. "Pure races" would eventually result, and any individual would in a very real sense be a child, not only of his parents, but of his race as well. Statistical averages would give an adequate description of the "race" as a population.

It is, however, known that heredity is transmitted, not through mixible "bloods," but through discrete and segregating genes. Hereditary variability, in a large sexually reproducing population, decays slowly or not at all. Children inherit only one half of the genes present in either parent. The genotypes of brothers and sisters (identical twins excepted) are different from each other and from both parents. In view of the large number of variable genes and the considerable number of alleles each of them is capable of producing, the variety of genotypes that may be formed in a population is so enormous that the genotype of any one individual living or having lived is unlikely to recur in any other individual. No groups of individuals with identical hereditary endowments can be formed. The concept of "pure race" in sexually reproducing organisms is absurd.

The use of statistical averages to describe the presumed ideal racial types logically implies the existence of "pure races." This method of desorption may suffice for cataloguing the major racial subdivisions, but it is completely misleading when applied to analyse the nature of races. Races are populations of a species differing in frequencies of certain variable genes. Scientifically rigorous description of races can be done only in terms of gene frequencies encountered in the populations.

Since the transition from race to species is gradual, it may be asked why the methods of studying the former must be so different from those used for the latter. The answer is that the quantitative differences between races develop step by step to become qualitative species differences. Accordingly, the classic methods of racial studies have a semblance of validity, when applied to very distinct races that are about to become species. The same methods are invalid where less differentiated races are concerned. Finally, attempts to discriminate between racial types supposedly coexisting within a single panmictic population are patently fallacious, unless the presumed "racial type" is the product of a single gene difference or the population is a race mixture of a very recent origin.

Techniques and methods of race studies based on estimation of

gene frequencies are rapidly developing. The remarkable investigations on the blood groups in man (recently ably summarized by Boyd) may be quoted as an example. The best known gene is that responsible for the four earliest recognized blood groups O, A, B and AB. This gene has three easily distinguishable alleles: A, A^B and a. Except for certain Indian tribes, which are supposed to be uniform in their blood composition, all human populations were proved to contain the three gene alleles. The frequencies of these genes are, however, by no means uniform throughout the world. Thus, the gene A^B is rather rare in Europe, frequent in Central Asia, and again relatively rare in Australia. The gene A is common in Australia but reaches a low frequency in Western Europe, e.g., in Ireland. Certain Indians (especially Blackfeet) have an exceptionally high frequency of A. A situation similar in principle is presented by the geographic distribution of the gene arrangements in the third chromosome of *Drosophila pseudoobscura* and in some related species.

It appears that the study of the geographic variation of a single human gene, that of the blood groups, is capable of giving more insight into the nature of the human races than decades of mensuration and computing racial averages. Yet, human populations differ assuredly not in blood groups only. There are hundreds, and possibly thousands, of variable genes in man. Concerted efforts to isolate and to study the distribution of these genes must be made. This is, admittedly, a slow and difficult task. Its slowness and difficulty does not justify, however, formulation of conclusions based on faulty data. The methods used by anthropologists so successfully, in the past, have not completely outlived their usefulness. The limitations of these methods must, however, be understood.

Systematists in most groups of animals and plants cannot be expected to engage in experiments necessary to reveal the gene elements of variation in their materials. Such a project would be clearly impractical, and, fortunately, it is not necessary. Systematists are bound to continue their important and fascinating studies on the collections of specimens preserved in museums.

But they can profit by taking the basic concepts of modern genetics into account in their daily work. For certain refinements in their techniques suggest themselves.

Many experiments on hybridization had been done before Mendel. Some of the early experimenters (Kölreuter, Gärtner, Naudin and others) have come within a hair's breadth of the discovery subsequently made by Mendel. The essential difference between their approach and that of Mendel was (we see it clearly in retrospect) that the latter made quantitative studies on the behavior of *separate* characters in the hybrids, instead of trying to classify the individual hybrids according to the *combinations* of characters. Now races differ from each other mostly in several, and, very frequently, in many genes. Genes are discrete and often independently varying units. There is some exchange of genes going on between most races of a species, unless the races are completely isolated geographically. Any systematist with a broad experience in racial studies knows that different characters distinguishing races may, to a certain extent, vary independently. Take the human populations inhabiting the continent of Europe. The stature, the cephalic index, and the pigmentation are only weakly correlated. Anthropologists and systematists will profit by centering their attention on the distribution of separate, well chosen characteristics in the populations that they examine. Discrete, alternatively variable characters are especially valuable. There is certainly no one-to-one correspondence between genes and characters. "Characters" are abstractions which we conceive more broadly or split up into their apparent components, as the exigencies of our studies may demand. Such "characters" in man as the cephalic index and the stature are obviously determined by many genes. They are genetically complex. Nevertheless, a character may be less complex than the organism as a whole. A character is merely an outward sign of the genetic mechanism, which, in conjunction with the environment, determines it. Hence, by studying the behavior of the variable characters, one-by-one, we make the nearest practicable approach open to us, without actual genetic experimentation, to examina-

tion of the genic variation. It is a sort of a short cut, a way to make reasonable inferences and approximations. Methodologically, it is similar to the inferential judgments used by systematists to distinguish races from species.

At present, many systematists are preoccupied with the detection of racial or subspecific "types" to which they can attach Latin names, valid under the priority rules. This procedure, patterned entirely on the methods of describing species, genera, and other groups, should be retained for cataloguing purposes. The study of the behavior of separate characters should supplement, rather than supplant, the classic procedure. As pointed out above, the racial "types" are really fictions. But these fictions become more and more useful pragmatic devices, as the differentiating races approach the species; less and less tenable, as we descend in the scale of racial separation. This is because the racial gene complexes become more and more differentiated *as complexes*, when a race segregates itself from other races composing the species. Few biologists have doubted the real existence of the negro and the white races, despite the presence of numerous intermediates between the two, and despite the fact that these races are very far from the degree of separation which could conceivably be deemed to be specific. On the other hand, the distinctions between the Nordic, Alpine, and Mediterranean races are uncertain, despite the efforts of numerous anthropologists. The exchange of genes between negroes and whites was, until recently, limited to a relatively narrow geographic zone. The exchange of genes between the subdivisions of the white race was, and is, extensive. It is quite probable that some genes of the paleolithic man persist in certain European populations, but it is misleading to label individuals possessing certain facial features as belonging to a race of paleolithic survivors. Only in terms of the distribution of gene frequencies, shall we be able to make sense of the differences between local human populations.

It seems not too much to ask of the anthropologists and the systematists that they remember that the achievements of genetics are quite pertinent to the subject matter of their studies. Surely,

this reminder works both ways. Geneticists are frequently at fault in their ignorance of the attainments of the descriptive biological disciplines. United, we shall make good progress. Divided, we run the risk of directing our work into the blind alleys of esoteric specialization.

SECTION OF PSYCHOLOGY

JANUARY 19, 1942

LOIS BARCLAY MURPHY, Department of Psychology, Sarah Lawrence College: *Patterns of Spontaneity and Constraint in the use of Projective Materials by Pre-school Children.* (This lecture was illustrated by lantern slides, photographs and paintings.)

In the course of my studies of sympathetic behavior in pre-school children in 1932–34, I found that most pre-school children, who were free to respond spontaneously, were sympathetic to situations which they understood, while the children who had low scores for overt sympathetic responses tended to be those who were also inhibited in other aspects of overt behavior normal to to children at that age. This drew my attention to the problem of spontaneity as such. I also found that children of a lower-income group at a day nursery were more spontaneous in both affectionate and aggressive behavior than children at the university nursery school, where the main study was made. This focussed my interest on questions of how the "family experience" patterns the development of spontaneity and control in different children.

During the last five years at the Sarah Lawrence nursery school, I have been studying this problem along with some others, using a variety of materials; chiefly paintings, finger paintings, Rorschach records, blocks, play with sensory materials—both visual, tactual, and auditory, and miniature life toys. Children were never coerced into participating in the experiments. Whenever possible, the experiments were terminated by the child. They averaged about 45 minutes in length. Since the purpose was research, not therapy, activity of the experimenter was limited to giving responses only when needed for rapport or for clarity of the record. The discussion is based on certain aspects of the records of the toys, finger-paintings and cold cream. The toys included five boxes with materials for house play, a village,

farm animals, wild animals, and soldiers. Each group of materials had been suggested by activities of the children during their free play. Both stenographic notes of behavior and photographs of structures were made as a record of the child's activity.

If we inspect the records of free play with miniature life toys from the point of view of spontaneity and constraint, the following main types of play with the toys were easily distinguishable: (1) exploratory play, when the child only names, inspects, or handles the toys in a "getting-acquainted way"; (2) arranging a static pattern with the toys, either of a rigid and meaningless sort, or a conventional representation, such as a house; (3) organizing the toys from moment to moment in various ways, which serve as a vehicle for fantasy of the moment.

The way in which the child plays with the toys reveals his feeling about various everyday experiences: a bed is a soft cuddly place that you enjoy, to one child; to another it is where you are sent when you are bad.

The toys were used in two ways, i.e., with the addition of sixteen large blocks from the nursery school, and without these blocks. Patterns of freedom and constraint (in addition to the usual differences of tightness, looseness, solidity, openness), appearing when the blocks were used with the toys, included the following:– enclosures with much or little room to move around in; high or low barriers (in one case, a double barrier around the enclosure within which the child played kitten); enclosures with large openings or small ones, with few openings or many, closed on the top or open on the top.

I have listed these to give some basis for comparison of normal, relatively unselected children with children in clinic or therapeutic situations. Fears, aggressive wishes, conflicts with parental authority are acted out along with fun-times. It is only the anxiety, which leads to preoccupation with one persistent theme, that is a source of inhibition of spontaneity.

Anxious repetition of the same theme is a less common form of inhibition in this group, however, than the constraint evident in rigid structures such as tracks, fences, and other enclosures;

mechanically set-up houses, made in the form of a square or oblong with exact lines rigidly adhered to. Structures of this sort are made without any evidence of spontaneous play before, during, or after the structure is produced, by children who lack freedom to express fresh ideas or feelings, but do not necessarily show motor inhibition. Motor-inhibited children simply do not play with the toys at all, on initial sessions, or until they are released from motor inhibition.

Without attempting to give any complete analysis of finger-painting, the following most obvious patterns are illustrated by the paintings presented here:– freedom or inhibition in choice of color; use or mixing of color; movement; contact, or texture responses. The free child uses any or all colors, is not afraid to mix them to make fresh nuances. His movements are varied, non-symmetrical, non-repetitive. His contact may show everything from rubbing, patting, smoothing, wiping, to scratching and slapping the paper. Different areas of constraint lie back of such differences, as being afraid to use certain colors, or afraid to mix them; making repetitious vertical or horizontal movements or only symmetrical ones; or only rigid patterns such as fences or walks. Few children of course are completely free or completely inhibited. The sequences and patterns of spontaneity and inhibition are of chief interest.

Over the period of three years, during which many of the children were observed, some children showed long-time sequences from markedly constrained patterns in painting to markedly spontaneous use of paint. Others began spontaneously and became more constrained as time went on.

In numerous cases, it was possible to trace direct connections between changes in the family and changes in the spontaneity-constraint patterns of the children.

In many cases, the same basic spontaneity-constraint pattern appeared through several media. Differences from one medium to another may appear and point to the desirability of finding a basal line for each child in reference to spontaneity, by discovering in which medium he is most free. Forthcoming papers will dis-

cuss these differences in "areas" of spontaneity in relation to the temperament of the child. Some three year olds are most spontaneous with sensory materials. Others are most responsive to situations calling for large-muscle activity. Others are most spontaneous with materials which can be manipulated manually, while still others are freest in dramatic situations, and still others respond most to music. Lack of spontaneity in one area, such as building blocks, or participating in group play, does not necessarily mean that the child lacks spontaneity generally. Different combinations of such areas appear to be related to temperament and abilities, as well as family experience.

My discussion so far, has been entirely on the level of description of simple objective differences, easily visible by an untutored observer of paintings and photographs of structure produced by the child. At another level, we may consider the use of symbolization by this group of normal children. Certain objects were used with different meanings by different children.

The fact that, at this age, symbolization of this sort often represents, in itself, an evidence of inhibition (that is, fear of dealing with a situation directly) is suggested by the fact that numerous children begin with symbolic handling of aggression and become progressively more open.

We find the following expressions of inhibition or constraint:- refusal to use certain materials, colors, or ideas; being limited to constrained patterns with toys, blocks, paints; indirect handling of conflicts or wishes by subtle forms of symbolization; and preoccupation with a certain idea surrounded with anxiety. Sequences and patterns of spontaneity and constraint may directly reflect contemporary areas of freedom and constraint in the family life; or they may reflect a distillation of sequences of experience through periods of years or shorter periods. These cultural sources of the child's patterns of spontaneity and constraint cannot be separated from the child's own sensitivity and adaptive patterns. Given similar external pressures or constraints, different children develop different patterns of spontaneity and constraint, depending upon their different temperaments and abili-

ties. Further studies will deal with the relation between these experiments and the Rorschach records of the children, and with the relation of pre-school patterns of spontaneity and constraint to the children's later development.

SECTION OF PHYSICS AND CHEMISTRY

JANUARY 9 AND 10, 1942

Conference on "*Protein Hormones of the Pituitary Body.*"

The Section of Physics and Chemistry held a Conference on "Protein Hormones of the Pituitary Body," as the second in the series for the Academic Year of 1941–1942. Doctor H. B. van Dyke, Squibb Institute for Medical Research, New Brunswick, N. J., was the Conference Chairman in charge of this meeting.

The program consisted of the following papers:

"Introduction," by H. B. van Dyke.

"Criteria of Purity of Proteins," by Theodore Shedlovsky, Rockefeller Institute for Medical Research.

"Hormones of the Posterior Lobe of the Pituitary Gland," By George W. Irving, Jr., Rockefeller Institute for Medical Research, and Vincent du Vigneaud, Cornell University Medical College.

"The Follicle Stimulating Hormone of the Anterior Lobe of the Pituitary Gland," by Bacon F. Chow, Squibb Institute for Medical Research.

"The Luteinizing Hormone of the Anterior Lobe of the Pituitary," by H. L. Fevold, Harvard Medical School and Harvard University.

"Lactogenic Hormone and Mammogen," by Abraham White, Yale University School of Medicine.

"Anterior Pituitary Hormones Affecting Growth and Metabolism," by C. N. H. Long, Yale University School of Medicine.

THE NEW YORK ACADEMY OF SCIENCES

announces

THE A. CRESSY MORRISON PRIZE CONTEST FOR 1942

Two prizes of $200. each, offered by Mr. A. Cressy Morrison, to be known as the A. Cressy Morrison Prizes in Natural Science, will be awarded at the Annual Dinner, December, 1942, for the two most acceptable papers in a field of science covered by the Academy or an Affiliated Society.

CONDITIONS:

(1) Eligibility. Membership in good standing of The New York Academy of Sciences or one of the Affiliated Societies, prior to submission of the manuscript.

(2) Date. Papers are to be submitted on or prior to November 1, 1942, to the Executive Secretary of The New York Academy of Sciences at The American Museum of Natural History, Central Park West at 79th Street, New York City.

(3) Papers. All papers submitted must embody the results of original research not previously published. The manuscript shall be typewritten, in English, accompanied by all necessary photographs, drawings, diagrams and tables, and shall be ready for publication. Papers must be accompanied by a summary of the data presented and conclusions reached.

(4) Awards. The awards shall be made by the Council of The New York Academy of Sciences. If, in the opinion of the judges, no paper worthy of a prize is offered, the award of a prize or prizes will be omitted for this contest.

(5) Publication. The Academy shall have first option on the publication of all papers submitted, unless especially arranged for beforehand with the authors, but such publication is not binding on the Academy.

(6) Wherever and whenever published, the papers awarded

the prizes shall be accompanied by the statement: "Awarded an A. Cressy Morrison Prize in Natural Science in 1942 by The New York Academy of Sciences."

Such statement in substance must also accompany any formal publicity initiated by the author regarding the prize paper. If published elsewhere, six copies of each prize paper must be deposited shortly after publication with the office of The New York Academy of Sciences.

THE NEW YORK ACADEMY OF SCIENCES,
Central Park West at 79th Street
New York City

DUNCAN A. MACINNES,
Recording Secretary

NEW MEMBERS

ELECTED JANUARY 22, 1942

ACTIVE MEMBERS

Archibold, Reginald M., Ph.D., M.D., Biochemistry, Assistant Resident Physician, Hospital, Rockefeller Institute, New York, N. Y.

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Bakst, James, Ph.D., Instructor, Dept. of Music, Brooklyn College, Brooklyn, N. Y.

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Gilvarry, John J., B.S., Research Assistant, Dept. of Physics, Princeton University, Princeton, N. J.

Gottfried, Sidney Paul, Ph.D., Assistant Chemist, Biochemistry Laboratory, Brooklyn Jewish Hospital, Brooklyn, N. Y.

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Lagomarsino, John A., B.S., Histologist, St. John's Long Island City Hospital, N. Y.

Moore, Stanford, Ph.D., Biochemistry, Assistant, Rockefeller Institute, New York, N. Y.

Mossman, Dorothea R., Ph.D., Assistant Professor, Dept. of Chemistry, Brooklyn College, Brooklyn, N. Y.

Natelson, Samuel, Ph.D., Research Chemist, Pediatrics Research Laboratory, Brooklyn Jewish Hospital, Brooklyn, N. Y.

Philip, B. Roger, Ph.D., Acting Head, Dept. of Psychology, Fordham University, New York, N. Y.

Smadel, Joseph Edwin, M.D., Medicine, Associate in Medicine, Rockefeller Institute, New York, N. Y.

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STUDENT MEMBERS

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